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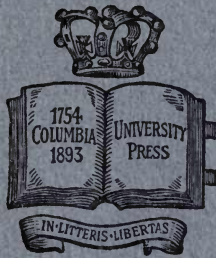
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ANALYTICAL CHEMISTRY AND ITS FUTURE

By

WILLIAM FRANCIS HILLEBRAND, PH.D.

Chief Chemist of the Bureau of Standards
Washington, D. C.



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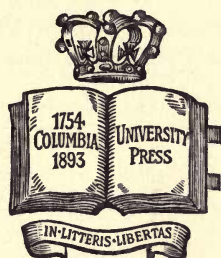
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OUR ANALYTICAL CHEMISTRY AND ITS FUTURE ¹

IN an address² read at Philadelphia nearly twelve years ago, I gave expression to some thoughts on the condition of analytical chemistry in our country as the condition appeared to me then to be. Those thoughts were based on an experience of many years, during which I was engaged wholly in analytical work of a more than ordinarily exacting nature, and especially upon observations that had been acquired in connection with several series of coöperative analyses of diverse materials. Since then my attention has been no less given to analysis, largely for the past eight years in a supervisory capacity, however, and I have had opportunity to note the conditions that now prevail with respect to chemical analysis and what an important bearing exact analytical work often has on problems of physical and electrochemistry, metallurgy, etc.

It seems to me then that I can choose no more fitting subject for my present discourse than a continuation of one so closely related to my life-work, one in which I feel a deep interest and of which I may be presumed to have knowledge somewhat worth presenting on an occasion like this. Then, too, since my remarks will apply most directly to analysis as it concerns the producers of the raw materials and the users of the products of applied science, the subject is eminently a proper one for the

¹ To several of my colleagues in the Bureau of Standards, to whom the first draft of this address was submitted, I am under obligations for suggestions that have been most helpful in its further elaboration.

² J. Am. Chem. Soc., Vol. 27, p. 300 (1905).

present occasion, and particularly so in an institution where applied chemistry made one of its important starts in this country, in the old School of Mines, with which the name of Chandler is so inseparably connected.

Although I shall cover now some of the ground traversed in my address of twelve years ago, in briefly alluding to the conditions of analytical chemistry in the present year, 1916, there is much to be said in developing one or two of the ideas then simmering in my mind and other phases of the general subject not then mentioned. So my subject calls for a more unrestricted title than I gave it at that time, and I shall speak to you of our analytical chemistry and its future, purposely restricting myself to a consideration of conditions as they exist and may become in this country.

In the early days of chemistry there was needed a vast accumulation of observations to serve as foundations for the development of the science. At the very basis lay the need for knowledge of the composition of all kinds of matter. Hence, it came about that many, if not most, of the great chemists of the time were of necessity analysts, and the analytical branch of chemistry stood in high repute. That this condition did not maintain itself, that chemical analysis during the latter half of the past century fell from its high estate and came to be looked upon more or less as a handy tool for ulterior ends, a tool, moreover, which need not for most purposes be of the sharpest or the best, or entrusted only to the most careful and skilled operators—all this has been recognized and lamented by many. The reasons for the fall are also well enough known and need not be discussed at length, but brief reference to some of them will be needed in view of later remarks.

Chief among the reasons for the neglect of analytical

chemistry is the enormous development, first of organic chemistry and later of the so-called physical chemistry. The effect was brought about in two ways: (1) by mere displacement, as it were, owing to the far greater promise of new discoveries, however commonplace, or because of the strong interest attaching to new and unexplored fields of inquiry; (2) by the unfortunate fact that for a long period approximate analytical results were thought to suffice in most of the industries, and even in scientific researches. This meant that slipshod work and methods came more and more into use, and less fundamental knowledge of analysis seemed to be demanded of chemists—all of which reacted unfavorably upon the standing of the analytical profession, tending to discredit it as a whole, even though it held members fit to rank with the illustrious pioneers.

In addition, some chemists came to feel that the field was an exhausted one, offering little to reward the research worker. How little this is true the events of recent years have abundantly shown. The growing sense of the important influence of small, even minute, amounts of this or that element or combination in a given material, and the high value of many ores and commercial products, has led to more critical examination of the methods used for determining the content of the substances in question in order to ascertain with greater precision the value of those materials, just as had been done long before for the precious metals, gold and silver. Such examination revealed not infrequently unsuspected defects in methods regarded hitherto as reliable and accurate, and that good results were due often to compensation of errors or were to be had only within a narrow range of conditions. One good effect of such investigations has been to make conservative analysts distrustful

of all new methods and less reliant on some of the old ones until their worth and suitability have been put to far more crucial test than was formerly deemed necessary.

Yet notwithstanding improvements made in important methods through painstaking research, it is evident that many methods will require a study differently directed or more profound than any yet made before light enough to meet even our immediate needs is thrown upon them. And who shall say what needs another century or even decade may bring forth? Are we not again and again even now confronted with the need to determine smaller and smaller amounts of a component and to make more and more perfect separations in order that the first may be possible? Are we to assume that a limit has been reached?

Another fact shows how untrue it is that the field of chemical analysis has little new to offer. Few ever thought, not so long ago, to look for the rarer elements in an ore or industrial product made from the ores. No use whatsoever was made of certain elements that are now serving most useful ends, either by themselves or in combinations. There are other elements, still chemical curiosities, for which no use has yet been found. Is there any more reason to believe for them than for the others that uses will never be found? Rare though they be, like gallium, indium, and germanium, and costly their extraction, the finding of a use for them will broaden the search for their ores and lessen the cost of production. With use will come a demand for methods of separation and determination, which must be accurate because of the small percentages in question or the enormous value of the material.

But there are other fields in which the chemist has to look to analysis of the highest order for help in solving

his problems. For instance, the importance of exact analytical methods in connection with physico-chemical researches is very great and is, perhaps, best illustrated in the preparation of pure materials. There is no question that physical constants, even atomic weights, have been determined, not infrequently, upon materials of doubtful or at least unproved purity. The practice is all too common of assuming that a certain number of crystallizations or distillations is sure to yield a product of highest purity. Conclusive results can be obtained only when methods are devised and applied by which the amounts of any possible contaminant present can be proved to be without influence upon the results sought. A single instance, borrowed from the experience of the Bureau of Standards may be of interest. In the preparation of pure alcohol to be used in the determination of a series of densities, tests were devised or confirmed for detecting the presence of minute amounts of ether, aldehyde, methyl alcohol, and water. The most delicate test for the latter was found to be the critical solution temperature of mixtures of kerosene and the alcohol to be tested. By this means the presence of 0.001 per cent. of water in the alcohol could be readily detected.

In the field of electrochemistry there is a similar need for exact analytical data. In the determination of the electrochemical equivalent of silver, from which the value of the ampere is derived, researches extending over several years have shown that the purity of the electrolyte is of fundamental importance. Thus, it was found that the presence in the electrolyte of the amount of organic matter derived from filter paper by the passage through it of the distilled water used, was sufficient to cause an appreciable effect upon the structure and weight of the silver deposit. In this case delicate analytical proced-

ures were devised for detecting minute amounts of such contaminants. In the same research the study of the magnitude of possible occlusions in the silver deposits has involved the use of painstaking analytical methods at the Bureau and elsewhere.

Similarly, it is believed that the securing of accurate information regarding the operation of commercial baths for electrodeposition will depend largely upon the application of exact analytical methods. Thus, preliminary observations have shown that very slight differences in the neutrality of nickel baths may produce great effects upon their operation. Here the application of the hydrogen electrode as an analytical tool will probably be of service. The great number of empirical observations regarding the effect of addition agents in plating baths will become intelligible only when means are found and applied for determining quantitatively minute amounts of the addition agents (for instance, one part per million of glue) or of their decomposition products.

The application of some of the concepts of the modern theoretical chemistry has helped much to a better understanding of the limitations of some common methods, of how to reduce the errors of one or another of them within more or less acceptable bounds, and of why others are not open to improvement. The same principles applied to the development of new methods will, it is to be hoped, lead more quickly to success than in the past, by enabling the discoverer to take account from the start of earlier mistakes or omissions and thus avoid the wasted effort that has been all too common.

At this point it may not be amiss to point out certain criticisms that apply to many new methods as first published. Almost no new method that has been proposed has been so rigorously worked out as to show all or nearly

all of its limitations. Generally the start is with the presumably pure single substance and the amounts operated upon are of considerable magnitude and do not cover a wide range of weights. This is not so serious a defect as to omit trying out a method that involves separations from other substances under a wide range of conditions as to relative and absolute amounts of the elements or compounds in question. A whole list is often given of results obtained in presence of other elements, but almost always the amount of the substance sought is considerable. No light is shed on the value of the method when that substance is in very small amount and the other greatly preponderates. Nor, in too many cases, is any proof afforded that results apparently good are really good and that more or less serious compensating errors are not involved. The consequence is often, as I have said, that one cannot take new methods at their face value or proceed to apply them under any and all conditions. They must first be more critically examined in order to complete and round out the work that was neglected. How this can be done will be discussed later.

What I have just said is not to be taken as necessarily reflecting upon the deviser of the incomplete method, nor need it deter others from trying to originate new methods or to improve old ones. There will be and must always be road-breakers and pioneer surveyors. Some fertile minds are fitted to make brilliant reconnaissances and unfitted for the laborious working in of details. Both types of chemical workers are needed. The former will still find ample opportunity for flights of invention and there will be no lack of room for the able and painstaking delver into the depths.

In the address already alluded to and elsewhere I dwelt upon the unsatisfactory condition in which the art

of analysis had been shown to be and expressed the conviction that our educational institutions must bear a large share of the blame in the matter. The faults which might be chargeable were perhaps more often those of omission than of commission, but I was able to point out no certain or even likely way which might lead to a better future. I think it may be worth while to reproduce certain paragraphs, with slight rearrangement, to serve not only as groundwork for what is to follow, but also as possibly suggestive leaders to those of you who are or expect to become analytical chemists or teachers of analytical chemistry.

"Many inquiries addressed to the participants in one series of analyses elicited the information that few knew anything definite about the quality of the water they were using, though examination showed it to be bad in a few instances and on the border line in others. Still less was known as to the quality of the reagents, except that they came from reputable firms. One admitted that a flaky sediment showed in his ammonia bottle, but he used only the clear liquid above. If the sediment represented silica from the bottle, as it may well have done, what had become of the other constituents of the attacked glass unless they were in solution?

"Now why were these things possible unless because it had never been sufficiently impressed upon the analysts in their student days that without proper tools to work with, among which water and reagents are first to be considered, good work is impossible? You doubtless do not fail rightly to tell them that absolute accuracy is unattainable in analysis, but do you make it plain that approximation is possible and that it will be the closer the greater the care bestowed upon the tools and at every step of the analysis itself? Is a student ever required

to find out by actual test how good his water is and both the kind and amount of its contamination, if such there be? Is it customary to instruct him in the testing of his reagents and as to the character of the contaminations to be looked for in all of the more important ones, or is he allowed to go forth with the impression that the label C. P., while not a flawless title, is a sufficient guarantee for all the demands of technical analysis? Is he, in fact, ever cautioned to find out, by actual test, the errors with which his work may be affected, due to imperfections in his tools of the kind just mentioned? And that without such knowledge and the ability to make correction for the defects, or the courage to fight for better materials with which to do, he will occupy a false position with respect to himself, his employers and the community at large?

"Is the student's work ever checked against material of which the exact composition is known? I do not refer here to such things as simple salts, but to more complex bodies like limestone, cement, zinc ore or slag, in which many separations have to be made and all constituents should be determined. Is the student in such analyses religiously required to test the purity of his precipitates and the completeness of his precipitations by a careful examination of the filtrates? And is he taught that a satisfactory summation does not imply correct separations? Or that closely agreeing duplicates are not proof of good work?

"Only by such exercises can the young worker gain any knowledge as to his own power to do good work, and acquire that proper confidence in himself which is so essential.

"My experience of the past few years has convinced me that in these respects, at least, much is neglected that

should not be neglected in the curricula of our colleges. It seems to me that if instruction in such fundamental essentials is not thoroughly drilled into the budding chemist, so that it becomes for him as much a matter of course afterwards to look to the quality of his tools as it is to weigh out his sample before analyzing it, he has received a scant equivalent for his years of study, and that he has good grounds of complaint against his alma mater if he comes to grief by reason of her neglect."

To the foregoing reasons for poor results may be added the youthfulness and inexperience of most of the instructors in quantitative as well as qualitative analysis. There must be young instructors, of course, but one of the rules which should hold for the young child in the Kindergarten or Montessori school ought to hold here too, namely, that the work should be led by or at least most closely controlled by one of experience and authority and of sympathetic insight.

If the conditions which I have sketched were true twelve years ago the question will be asked, and quite naturally: Have they improved? Candor compels me to say that evidences of improvement are few. In certain lines of work there has been some bettering of conditions, but we are still confronted with wide divergences in almost every direction between the results obtained by different analysts upon the same sample. I have many opportunities to note this fact in the coöperative work which is done upon the samples which the Bureau of Standards issues as standards for checking the skill of analysts or the value of methods used in industrial laboratories and educational institutions. The fact is further emphasized by the numerous requests received at the Bureau for umpire assays to settle the differences between commercial analysts, and still further by the comparative lack

of sound or comprehensive knowledge of analysis among the young men who come to us from the colleges and universities.

While I believe now quite as strongly as I did twelve years ago, that our educational institutions are not doing their full duty by their students in respect to the analytical branch of chemistry, I see no way by which the situation is to be relieved much under the prevailing educational system. Classes are too large and competent instructors too few for that individual oversight which is so essential to the attainment of the best results, and too little time can be devoted to analysis by students because of the multifarious demands made upon them in other directions. The conditions are far different from what they were, for instance, in Bunsen's laboratory, when students averaged two or three lectures a day and ambitious ones put in nearly all the rest of six days a week in the laboratory free from the harassing incubus of impending term examinations. This comparison is not made in a spirit of complaint, but merely to contrast ideal conditions, that have probably passed away even in Germany, with those which we have to face.

If the defects noted are to be corrected in a measure, I see no way by which to accomplish this except by increasing the number of teachers, and by extending the period of academic instruction, especially for those who contemplate following the most intricate art of analysis professionally and not merely as a prelude to something else which may bring greater financial returns. The adoption of this course, so often suggested, seems rather remote, yet it should certainly be adopted if we are ever to have a really competent body of instructors. As instructors they would enter upon their teaching career with better preparation and wider experience, but this alone is not

enough. Time should be accorded them, while teaching, to gain further experience in all manner of analytical procedures and to keep abreast of the advances made in the art of analysis. The benefits that would follow such changes would soon be manifest, but I doubt much if the exacting requirements of a great deal of our analytical practice would be fully met by them, so additional aids would still be welcome.

One of these aids we now have to a limited extent. I refer to the use of standard samples, such as are issued by the Bureau of Standards, following the practice for some time in vogue in the iron and steel industry particularly. The larger number of these samples, as most of you know, have been analyzed by eight to a dozen chemists who are expert in a given field of analysis and the averaged results are supposed to represent as closely as may be the actual composition of the material as a whole or as to certain of its constituents. If a chemist is able to analyze one of these samples correctly it may be presumed that his technique is good and that the results he obtains on the same type of material of unknown composition are to be trusted. If not, then either his technique or his methods are at fault.

In addition to these analyzed samples, the Bureau of Standards has issued several single chemical substances (thus far only compounds of carbon) of the highest attainable purity, which serve as standards for volumetric or polarimetric analysis, or for calorimetry.

The value of such aids as these to the analyst has been very great, and not only in the ways indicated. In connection with their analysis, prior to issuing them, many interesting observations have been made upon the relative values of various methods in common use. Our experience at the Bureau of Standards, extending now

over a number of years, has shown that these methods are not all of equal trustworthiness, and that one or another of them is affected by hitherto unsuspected errors, either inherent in the method as practised or introduced by the presence of an unlooked for element. The errors so detected may be very small and for some purposes negligible, but their existence is a disturbing feature and one which is sometimes of moment. A few illustrations will suffice to make this point clear.

(1) There are several methods that are in daily use for the determination of phosphorus in steel. Most of them depend on the precipitation of the phosphorus as ammonium-phospho-molybdate and many are the articles that have been written bearing on the proper conditions for forming and washing the precipitate. The wash liquid used in the alkalimetric method, which is probably the most widely employed of any of the methods, is a one-per cent. solution of potassium nitrate. Work done at the Bureau of Standards recently has shown that the solubility of the phosphorus-bearing precipitate in this wash solution, even in the absence of vanadium, is considerably more than has been suspected. In fact, on a high-phosphorus product, if an attempt is made to wash until the washings are neutral, the result may be several hundredths of one per cent. low. This appears to be true whether the phospho-molybdate is precipitated in presence or absence of iron.

(2) The so-called evolution method for determining sulphur in iron and steel is one very much in vogue in industrial laboratories because of the short time required for the determination. In it the sulphur is driven out, mainly in the form of hydrogen sulphide, by boiling the alloy with hydrochloric acid, and collected in a solution of a cadmium salt with a view to determining the sulphur

by titration with iodine. The method suffers, however, from defects, some of which are understood but others not, as is evidenced by the very discordant results that are reported by different analysts upon the same sample. So far as can be determined the most discordant results are sometimes obtained by analysts who seem to follow precisely the same procedure. Evidently there is room here for some critical research.

(3) Manganese is determined in irons and steels by several methods, of which the bismuthate method affords perhaps the most concordant results in different hands when carried out according to certain closely prescribed details of manipulation and standardization of volumetric solutions. There is still doubt, however, as to just what the conditions should be for obtaining a correct end-point in the titration.

(4) The methods in use for the determination of tungsten in ferro-tungsten failed completely in a particular instance that was brought to my attention by the chemist of a large steel plant. Works and commercial analysts differed by several per cent. in their reported results. A partial explanation for the disagreement was afforded by finding columbium to be present in the alloy, an element hitherto unnoticed and unsuspected in such material. Attempts to devise a convenient and accurate method to meet the conditions have not been very successful, so far as I am aware. Here, again, is room for an interesting study.

(5) In the analysis of a brass, one of the Bureau of Standard's series of analyzed samples, somewhat discordant results for lead were reported by different analysts. It was found that those results which had been obtained by depositing the lead electrolytically on the anode as peroxide involved a slight error by reason of

coprecipitation of small amounts of silica and stannic oxide, these having been present presumably in the colloidal form in the solution as residuals from the separations that had preceded.

These examples show what a wide field there is for studies of a very refined character upon many of the methods that are in wide use before it can be said that we really know them.

Useful as standard samples are, those which are employed as actual checks upon analysis do not fully meet our needs. They do not tell the analyst wherein the defect of his method may lie, if defect there be. How then may we hope further to benefit the increasing body of industrial and commercial analysts, and instructors as well, who have no time for trying out methods of direct determination or of separation, the latter involving often more difficulties than the former?

Before attempting to answer this question I must take up the subject of standard methods of analysis. By a standard method is meant, in a restricted sense, one which has been put forth by a body of recognized standing, usually a committee acting under the auspices of some technical organization, for use in determining the value of a particular kind or type of material. This country has taken a decided lead in the direction indicated. The following quoted paragraphs are repeated from a paper yet to be published in full that I presented to the Second Pan-American Congress nearly a year ago.

"Most by far of the so-called standard methods are so by virtue solely of their having been put forth as such in a recommendatory way by some organization without imposing any obligation on any one to follow them. This condition takes away much of their standing in comparison with those methods of which the use is binding upon

any body of chemists. There are, again, those methods which have been proposed as standard by individuals and are generally without organized backing. It is thus evident that discrimination must be exercised in deciding which of the very many methods put forth as standard have any claim to be considered such.

“The main arguments against standardizing methods have been: (1) That the individuality of the analyst should not be suppressed; (2) That to make methods standard would tend to prevent their improvement and to discourage search for better methods; and (3) That a given method may be applicable to a given material only within a narrow range of its possible compositions, as illustrated by pig iron and steel.

“The answer to the first of these objections is that experience has shown abundantly that for commercial purposes good agreement among analysts is seldom to be hoped for unless each worker uses the same method in which every step and condition of the procedure has been minutely prescribed. It is for commercial purposes that standard methods are most needed, and they need not always be the most exact methods if the latter require the expenditure of time so great or the employment of apparatus so expensive as to defeat the end in view. For many umpire analyses and for those upon whose results no commercial transactions are dependent, the analyst has ordinarily full liberty of choice.

“The first part of the second objection rests upon a misapprehension. In the minds of those who were pioneers in the movement for standardization it was probably never contemplated that methods once adopted should endure beyond the life of their usefulness. Either the committees responsible for them would revise them as occasion demanded or new committees would under-

take their revision. This is the trend of intention here in the United States and undoubtedly wherever standard methods have obtained a foothold. It is, further, inconceivable that the establishment of standard methods should tend to discourage attempts to improve them or to substitute better ones in their place. If, in the opinion of its users, a method had become unequal to the demands it was expected to meet, considerable credit would accrue to him who should bring it up to the later requirements or secure the adoption of a better. Such incentive would always suffice to prevent stagnation in the stream of analytical progress.

"The third objection must be admitted to be well taken, although it suffices in no way to secure the condemnation of standard methods in general. The difficulty referred to under (3) above has been well covered by Dr. Dudley in collaboration with Mr. F. N. Pease in their paper³ read before the World's Congress of Chemists at Chicago in 1893. Their remedy—a partial one only—calls for employment of a variety of standard samples for a given material, covering adequately its usual range in composition, of each of which samples the composition has been determined accurately. Iron and steel are examples of this kind. If then, by applying his method to the particular standard sample which lies nearest in character and composition (mineral as well as chemical) to the material he is to test, the analyst obtains correct results, he is usually justified in concluding that the results he gets in the ordinary course of work upon similar material are also correct.

"The use of standard samples does away in a measure with the need of standard methods, for any method that will yield the true composition of the standard is a suit-

³ Jour. Am. Chem. Soc., Vol. 15, p. 501 (1893).

able one for a given analyst. Even so, it is usually only when this acceptable method is employed with strict adherence to a certain procedure that it yields a correct result. Thus prescribed and limited by a competent authority it may become a standard method for use in all hands. Nevertheless, a standard method is not always applicable to a complex material throughout the whole range of its possible compositions. In such case modifications of the method must be used suited to the special conditions, or different methods altogether are called for. The determination of the need for such variations or changes is one of the duties of the committees entrusted with the choice or formulation of standard methods. These committees have also to consider, in special cases, the need of a refined method for umpire work, and of another less detailed for work of a more routine character."

The principles which should lie at the basis of a standard method have been stated by me in a paper on Standard Methods of Sampling and Analysis and Standard Samples,⁴ as follows:

1.— That its limits of accuracy and its applicability are clearly defined and understood.

2.— That it should yield sufficiently accurate and concordant results in the hands of different analysts.

3.— That it should not demand such close adherence to detail or such manipulative skill and judgment, or such time consumption as to affect seriously its usefulness.

4.— That it should have been tested upon material of high purity or upon material carefully analyzed by independent and reliable methods.

5.— That the results obtainable upon a given class

⁴ J. Ind. Eng. Chem., Vol. 8, p. 466 (1916).

of materials should not be too dependent upon the composition (steel, iron).

These principles, however, have been seldom rigidly observed as to the methods hitherto offered. Having been prepared as a rule by different bodies of men of varying caliber and judgment, and sometimes by compilation of existing data rather than by direct rigid experimentation and trial, the methods are of very unequal merit. No argument seems to be needed to support the assertion that the preparation of such methods should be entrusted only to men of much experience and sound judgment, who have ample time to devote to most critical study of them. Unfortunately, this last condition is one that it is seldom possible to meet. The consequence is that too often the results do not measure up to the high standard that should attach to such methods. Nevertheless, an admittedly imperfect method is better than none if its limitations are well understood, for it affords a better basis for fixing the value of many articles of commerce than a variety of methods of uncertain relative and even absolute merit.

There is another requirement, however, without which the best of standard methods will avail little, namely, reagents of uniformly high quality. The need for enforceable specifications for reagents has long been apparent and the American Chemical Society at one time endeavored to bring about improvement in the quality of reagents. It is unnecessary to go into an exposition of the reasons why comparatively little came of this attempt. Some promise of relief seemed to be afforded, however, by the appearance on the market of 'analyzed' or 'tested' reagents, the bottles bearing statements of the nature and amounts of impurities present, or that certain possible contaminants were absent. But it soon appeared

that these statements could not be taken at their face value without risk, and the situation is particularly deplorable just now. The following recent observations are worth recording.

In sodium carbonate purporting to be free from phosphorus and silica both were present in amounts which condemned the reagent utterly for a variety of uses. Sodium hydroxide was found to be very impure, in marked contrast with the statement on the bottle. Sodium sulphide contained not only much thiosulphate or polysulphide but was black with iron sulphide, although said to be free from iron. Lead chromate, alleged to carry 0.0003 per cent. of nitric acid, held 7 per cent. of lead nitrate.

No condemnation is too severe, even in the present times of stress, for the manufacturer who so mislabels his wares. We certainly need a new law or an extension of the Pure Food and Drugs Act to protect the chemist.

Committee work, then, as hitherto practised does not conduce to the production of results of consistently high standard. My experience makes me feel strongly that the advances to be expected through committees of professional societies are too often slight, and are secured at a cost of time and effort quite incommensurate with the gains. This is especially true because: (a) It is difficult to hold the necessary frequent conferences of members widely separated, from which it results too often that the approval of a society is given to what is practically one man's work; (b) the members of committees sometimes lack a real conception of what a standard method should be in the light of the criteria by which they should be judged; (c) if competent in this respect, the members are not often able to devote the needed time to actual and painstaking experimental work; (d) different methods

are in the hands of different committees, without direct control by some head which has authority and is competent to see and direct that the fundamental principles are observed and followed in each and every case, to the end that the results shall be uniformly of high grade and consistently reported.

These and other considerations lead to the conclusion that such work should be in the hands of a permanently constituted body of well-salaried men under a director of the very highest ability as an analyst and of excellent tact and judgment. These men should be appointed only with the expectation that they are entering upon their life-work. It should be the intention to retain at all reasonable cost the services of those who prove thoroughly acceptable. Acceptability includes of course not only analytical skill, but also diligence, absolute honesty, and the highest conception of the dignity of this work. The beginnings of such an enterprise would be beset with uncertainties and the output not always of the desired quality, but as the men gained experience and insight into the fundamentals of their general work, improvement would come and in the end there would be gathered in this institution a band of men of unrivaled experience and knowledge, whose recommendations would carry such weight that little hesitation would be manifested by technical societies and legislative and industrial bodies in giving them their formal and practical sanction.

But the field of work for an institution of this kind should not be restricted to the comparative study of different methods that are applicable to the determination of one or more elements in a given class of materials, such as irons and steels, brasses and bearing metals, but should include the preparation, analysis, and issuing of standard samples of many kinds, both commercial articles

for direct checking of analysis and pure substances and solutions intended for calorimetric, volumetric, and other uses.

Furthermore, and this is very important, the institution should not act independently of the many industries which it is established to aid, but should tactfully maintain the closest and most sympathetic relations with them all, inviting indeed coöperation of the most intimate nature. Such coöperation would be very valuable oftentimes in exposing difficulties, and thus indicating the directions which research in the central laboratory should take. It should include occasional visits to and from the laboratories of one side and the other for consultation and the actual carrying out of analytical operations, whereby misunderstandings may be sometimes cleared up and the causes of differences ascertained more readily than by any amount of correspondence.

The institution should also be a court of last resort in disputes as to the value of articles of commerce that are subject to chemical test, not only as to the correct chemical composition but also with regard to the causes for the differing results reported by others, and, in general, should be a clearing house of information and a source of inspiration in all that relates to chemical analysis.

Before taking up the next phase of my general proposition, it is desirable that the functions which the proposed institution should eventually exercise be clearly understood. At the risk of some repetition I will therefore summarize and for convenience group them under the three heads, Research Work, Referee Work, and Educational Work.

A. Under *Research Work* fall: (a) critical comparison of methods of analysis in use or proposed for different classes of materials in the light of the criteria already

presented; (b) improvement of existing methods when possible and devising of new ones if the old are inadequate; (c) recommendation of the methods found to be best suited for commercial needs, accompanied by complete and unambiguous details of procedure together with a statement of the accuracy ordinarily attainable; (d) determination of the causes of disagreement in the results of analysis of the same material by different analysts; and (e) preparation of specifications for reagents.

B. *Referee Work* covers: (a) the making of umpire analyses in cases of irreconcilable disagreement between different analysts; and (b) the preparation and issuing of standard samples for checking methods and the skill of analysts, for volumetric analysis, and for the calibration of instruments such as polarimeters and calorimetric bombs.

C. *Educational Work* involves: (a) coördination of researches in progress, so that the results when available will be better suited for intercomparison and therefore more useful; (b) assisting in the wider adoption of standards and methods already accepted and found to be satisfactory; (c) study of and recommendations regarding the best methods of teaching analytical chemistry; (d) preparation and publication of bibliographies upon analytical chemistry; and (e) answering questions of all kinds relating to chemical analysis.

I have purposely refrained from including methods of physical testing, for fear of overstepping the legitimate field of an institution intended for chemical research. However, it might be proper to consider eventually if many methods of physical testing that are ordinarily employed by chemists should not be regarded as falling within the scope of an institution of the kind proposed. In any event it should coöperate in the coördination of the results of chemical and physical testing.

At the present time much of the literature published as the result of researches and investigations is of such nature as to make the results of limited value, largely because the investigation was not suitably planned or the results adequately recorded and reported to permit of comparison of one worker's findings with those of others who have preceded him. A permanent body which could obviate such a large amount of wasted effort as is now evidenced would in this field alone accomplish a great good for our science. Many investigators would be glad to avail themselves of its advice and criticism, and in many cases the scientific results put forth from our educational institutions and private laboratories might, through the coördinating influence of such a body, be made of greatly increased value.

Let us now consider how such an institution might be brought into being. There occur to me but three ways: (1) through one of the existing scientific bureaus of the federal Government, preferably and naturally the Bureau of Standards; (2) through endowment and control independent of the Government; and (3) through private endowment under some form of federal trusteeship.

The first and second of these plans have their advantages and disadvantages, which will be now set forth.

In favor of the first is the prestige which governmental support and control would lend. Although on first thought it may not always be easy to see why prestige should attach to the decisions of a bureau of the Government in so much greater degree than to an independent establishment, that it usually does is a fact that has been many times borne in upon me.

The prestige attached to the Government is based on several factors, of which it is not easy to estimate the relative potencies. One is that the National Government

represents the interests of all the people, that it is disinterested, having no small group to serve exclusively, that it seeks the welfare of the lowly as well as the exalted, that owing to this sense of responsibility and accountability serious scientific work undertaken by the Government is usually conducted in a manner to command the respect of the scientific world. These two factors have supplemented, perhaps, a third, which is that the people regard the Government as a coördinating center having to do with matters of general concern, so that when the Government speaks it is usually not the opinion of an individual but with the authority of the entire Government and for the entire people. That the public looks up to its Uncle Sam as counselor and adviser is attested by the innumerable inquiries that come daily in his mail from all parts of the country. The conscientious attention paid to these inquiries has had no small share in molding public opinion.

The readiness with which all interests coöperate freely with Government agencies is especially marked in coöperative scientific research. As the essence of the proposed institution would be coöperation in fundamental chemical research the Government plan would present a strong case.⁵

Evidence that prestige attaches in the minds of chemists, as distinguished from the general public, to statements issuing from Government institutions is afforded by a very evident tendency toward the use of methods of testing that are employed by the Government. This tendency is particularly manifest with regard to methods that are used for checking up deliveries under specifica-

⁵ A slight disinclination is apparent to full and frank coöperative work where commercial or personal interests are involved. Such unfavorable tendency of private work has resulted in appeals to the Government to take up lines of work where conditions are more favorable.

tions. It is true that the desire to copy may not be based always on acknowledged superiority of the Government methods, but rather on the wish to reduce the probability of rejection of deliveries, by use on the part of the manufacturer before delivery of the same method that is to determine the question of acceptance or rejection. However this may be, the result tends to bring about in a very natural way a more general and desirable uniformity of procedure. If this is the effect, it follows as a matter of course that the Government laboratories should be awake always to the importance of maintaining and improving the accuracy of their methods and further that they should be afforded encouragement and every facility to enable them to live up to these requirements.

An additional argument for having this work done at the Bureau of Standards is the help that would come through its facilities in physics. The chemical institution proposed would need to utilize the standardizing facilities of the Bureau named for all measuring apparatus and to a considerable extent the optical work would have a material bearing upon chemical work, especially in the newer fields of spectro-chemistry, refractometry, and physico-chemical methods generally. It would seem undesirable to duplicate this expensive equipment if it can be made available for the chemical work under consideration.

It has long been my hope that at the Bureau of Standards we might gradually build up a section which should be the clearing house in all matters of the kind under discussion. We are frequently asked to settle disputes regarding the content of a given element or compound in a great variety of materials and to give information on all manner of analytical procedures. Our ability to grant such requests has been, however, far too limited from lack

of a sufficient number of specially trained analysts and the inability of those we have to spare the requisite time for such problems. These problems often involve a considerable amount of research, for our aim is not only to determine the true value of the material in question but also to ascertain if possible the causes of the differences reported by others. I regard this latter point as of even greater importance than the former because of its educational value. Under the present conditions of federal support I see little prospect of a full realization of my hope. The cost of upkeep of such a division would be out of proportion to that of other phases of the Bureau's activities, for its complete realization would be financially equivalent to establishing a new institution.

Other serious objections to support and control by the Government are: (1) the instability of a work that is dependent on annual Congressional appropriations; (2) the likelihood that at any moment in emergencies the men engaged may be called upon to assist in entirely foreign kinds of work; (3) the restrictions of the civil service regulations and the inflexibility of the salaries attaching to all statutory positions. The objections enumerated are very important, for without assurance of permanency and continuity of work the success of such a scheme as I have suggested would be highly problematical and its lasting or even temporary interruption when once established little short of calamitous.

The restrictions under the third objection make it difficult or impossible in the first place to select men for particular kinds of work, or to give timely promotions to deserving men, or even to appoint anyone unless a vacancy happens to exist in the grade for which a desired man is qualified. This lack of flexibility in the appointment of new men and the promotion of old and tried ones is one

of the most serious handicaps to efficiency in Government service. These statements are in no way to be taken as condemnatory of the principles for which civil service stands, for the situation in Government service, even with the limitations stated, is vastly better than it was before the Civil Service Commission was established, when political influence controlled appointments, promotions, and dismissals in no small degree. The particular objection that statutory salaries are inflexible might be met by the provision of a special fund, as has been many times done for other purposes. Such funds are less subject to salary restriction than those called statutory, but Congress has been wont to look upon them with disfavor. The objection that was based on the uncertainty of Congressional appropriations would not hold so much for a separate bureau or establishment as for the supposed case of merger with an existing bureau. This follows from the fact that the effect of Congressional action is far less likely to result in abolishing or even hampering seriously the work as a whole of an established Bureau than of one of its divisions.

In favor of independent endowment is the greater stability in a certain sense of an institution so supported and a greater freedom of action and choice of men, offset in a measure by its lower prestige. This latter weakness might hold more during the earlier years of its existence, for under proper guidance there would seem to be no reason why it should not in time merit and obtain much, if not all, that now attaches to institutions fostered by the Government. The greater stability alluded to lies, however, rather in a surer continuity of existence than in certainty that the institution will be able always to fulfill its functions in full measure. In times of stress or through unfortunate investments the income might be

much curtailed or even cut off entirely, contingencies which Government establishments may be less concerned about.

There is this to be said further in favor of independent endowment and control, that if the control were in the hands of the technical organizations most concerned chemists would come to regard the institution as more peculiarly their own child, in the growth and performance of which each might take pride but for whose conduct they themselves alone would be responsible. Certainly the time is ripe for chemists to be represented by a national institution which should serve for fundamental work of interest to all chemists the same function that private and industrial laboratories serve for their clients.

A compromise between these radically different suggestions for support and control remains to be considered, namely, through private endowment under some measure of federal control, either of the funds or the work. The Smithsonian Institution is a well-known example of federal administration of a private bequest with control of appointments by the Civil Service Commission. The relation of the Bureau of Mines to its work on radium and some other experimental researches is suggestive of how Government supervision of work might be made compatible with outside control of all expenditures except, perhaps, for the salary of the Government official in charge. If Congress could be persuaded to contribute a building and to locate it on the grounds of the Bureau of Standards, where its staff would be in close touch with men engaged in many fields of testing and research, physical as well as chemical, the conditions might be ideal for attaining the maximum of success. Such juxtaposition might fail in its aim, however, if salaries in the two neighboring laboratories had no common basis.

Stated a little differently this means that salaries for statutory positions under the Government ought to be materially increased, for as already intimated, I regard the payment of adequate compensation as vital to the success of my general proposition.

The kind, degree, and manner of control to be exercised by the Government would need careful consideration, particularly with respect to the choice of the staff, for if with any sort of Government control appointments could be made only through the civil service a serious hindrance to successful operation would arise.

Finally, the question must be asked: What is the likelihood of such an establishment ever becoming a realization? To this no answer is immediately at hand. Certainly nothing will come of the proposal if it does not meet with the approval of the best judgment among chemists, and then still nothing unless a popular sentiment in its favor is awakened, nourished and made to grow by persistent discussion. The needed impulse and the demand created, the rest should not be so difficult now that we are awakening to the importance of chemistry to the life of the nation. The interests of the great chemical industries, or of industries at whose foundations chemistry lies, could surely be depended upon to create, through endowment or influence upon Congress, an institute for analytical research which would be the first of its kind and which would help to place this country in the very forefront of progress among nations. The industries have received so much from pure science in the past without adequate return that the time seems ripe for them to contribute generously towards its further development, especially along lines which promise so much to themselves as those which I have indicated. It might even be that some private benefactor would arise to do

for chemistry what has been so magnificently done for astronomy, terrestrial physics, biology, and the study of the causes and prevention of disease. Given a properly prepared field I do not feel that I am over-optimistic, considering the wave of interest in matters pertaining to chemistry that is sweeping over the country. The preparation of the field should be intrusted to a carefully selected and energetic committee.

I have already intimated that, whatever plan might be adopted, the closest coöperation should be sought and maintained with the industries. Indeed, an advisory board of outsiders should be selected to assist the director in the planning and administration of the scientific work, and the connection of this board with the affairs of the institution should be very real and not nominal. The laboratory itself should be, however, in no way debarred from taking the initiative without reference to the board proposed. Indeed, the laboratory force would often be in far better position to know or learn the weak points in current procedure than any advisory body.

Here let me interject parenthetically a word on a subject that has been far too much neglected by chemists in the past. It seems that now is the time when they should bring pressure to bear to secure that share of official as well as private recognition which is their right by reason of the immense importance of their work to the general welfare. The relative ease with which hundreds of millions of dollars are obtained for military purposes offers a startling contrast to the almost complete indifference of the great scientific interests of the country to Government aid to science. Without raising controversy over the relative value of military and scientific preparedness it is certainly true that if the scientific societies presented their case for Government aid to

science with a fraction of the efficiency with which the case for national defense has been prepared, there is scarcely any ideal for such an institution that could not be promptly realized. It is time that chemists entered the field far more than they have done to show how they can contribute to the general welfare and what they need in the way of countenance and support.

It would be going too far afield at this early stage to propose even tentative details for the organization and conduct of the proposed establishment, which some of you will perhaps recognize as patterned in its objects and scope somewhat after the international institute for chemistry that Professor Wilhelm Ostwald advocated in great detail a few years ago.⁶ Ostwald had in mind an institute of international scope to cover the whole vast domain of chemistry. I propose one of more restricted and only of national scope. What might grow out of a well-established and successful organization in the United States with respect to international coöperation may be left for the future to determine. World-wide coöperation from the start would be ideal, but even under normal conditions the task would be herculean, as it is at present impossible of accomplishment. Let us therefore proceed with only our own immediate needs in view, in the certainty that if our initiative succeeds others will copy and that when the time is ripe we may expect to lead in bringing about the broad world coöperation that must eventually come.

⁶ Only after this address was prepared did that of Professor G. G. Henderson, President of the Chemical Section of the British Association for the Advancement of Science, Newcastle-on-Tyne, 1916, come to my attention. Professor Henderson's address contains observations and suggestions which parallel in a measure portions of my lecture, although without specific reference to analytical chemistry.

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